ROYAL ASTRONOMICAL SOCIETY.

Vol. XVI. *January* 11, 1856. No. 3.

Manuel J. Johnson, Esq. President, in the Chair.

The Rev. Wm. Russell Almond, Stapleford, near Derby, and
The Rev. George Venables, St. Paul's, Chatham,
were balloted for and duly elected Fellows of the Society.

Volume XXIV. of the *Memoirs* has just been published. Besides the usual matter, it contains an Index to the last fourteen volumes. The price is 7*s.* 6*d.* to Fellows of the Society, and 12*s.* to the public. Vol. XV. of the *Monthly Notices*, which contains shorter papers, abstracts of memoirs, and notices of various astronomical publications, is given to purchasers of the 4to. volume. The two publications are supplementary to each other, and are to be considered as parts of the same series. They contain scarcely anything in common, and between include a complete account of the proceedings of the Society during the year.

New Planet.

On the evening of the 12th of January, at $9^h\ 33^m$, a new planet of the 9.10th magnitude was discovered at the Imperial Observatory, Paris, by M. Chacornac. The following positions of it were obtained:—

	Paris M.T.	R.A.	Decl.	No. of Comps.
	h m s	m s	′ ″	
Jan. 12	11 52 43	.....	δ * −2 11·9	3
	12 18 22	α * +4 36·7	.....	2
13	9 54 32	α * +3 46·9	.....	4
	10 21 6	.....	δ * −2 37·6	3
	10 43 4	α * +3 45·3	.....	2

Position of the star of comparison (8th magnitude), according to a meridian observation of the 13th:—

R.A. = $8^h\ 35^m\ 19^s{\cdot}99$ Decl. = $+17^\circ\ 23'\ 53''{\cdot}5$

On the Rings of Saturn. By Professor Secchi.

(*Communicated by the Astronomer Royal.*)

"In an interesting memoir published in the *Proceedings of the Academy of Sciences of St. Petersburg*, vol. v. 6th series, 1852, M. Otto Struve has announced the suspicion that the rings of *Saturn* were in a state of gradual collapse, so that, after a certain interval of time, which probably might not be long, the planet would appear totally deprived of those interesting appendages. The suspicion is the more reasonable, inasmuch as nothing proves that the system is in a condition of permanent equilibrium, and since, not only the early observations, which might be liable to some doubt, but even the most recent observations, support the hypothesis. I extract from the memoir of M. Otto Struve the following table in support of this assertion, which will be found useful hereafter. I add to it the measures of Mr. Lassell for 1853 (*Astronomische Nachrichten*, No. 856):—

Table (A).

Observer.	Epoch.	Exterior Diameter of the Ring.	Diameter of the Planet.
		"	"
Huyghens	1657	45	18
Cassini	1691	45	...
Pound	1719	42	18
Bradley	1719	41·25	17·75
Rochon	1777	40·6	16·9
W. Herschel	1791	46·68	...
W. Struve	1826	40·10	17·99
Bessel	1831	39·30	17·05
Encke	1837	40·93	17·68
Galle	1838	40·90	17·91
O. Struve	1851	39·74	17·61
Lassell	1853	40·88	17·45

"The discordances remarked in these measures are sufficiently sensible, and I have thought that it would not partake of any rashness on my part to attack the question. After having acquired sufficient practice in measures of the planets with *Jupiter*, and having found my measures agreeing with those of M. Struve, I have availed myself of the last opposition and of the maximum opening of the ring, to execute a series of measures, not only of the ring, but also of the planet, so as to ascertain the degree of precision and confidence to which they might be entitled. Each number is the mean of two, and more frequently of three, double measures, which are very accordant, and of which the probable error of an individual measure does not exceed $0''{\cdot}2$. The measures have been executed only under an excellent condition of the atmosphere, except on two occasions, one of which was for the purpose of ascertaining the influence of less favourable circumstances. To the measures made during the last opposition I have

also added, for the purpose of comparison, those of the opposition of 1854–5. I have not corrected any of the measures for refraction nor for phase,— not that I considered the effects depending upon these causes to be insensible,— but because I perceived discordances of another kind of far greater magnitude, the origin of which I was desirous of examining before I calculated those small corrections.

Table (B).

Days of Observation.	Exterior Diameter of Exterior Ring.	Middle of Principal Division.	Interior Diameter of Ring B.	Interior Edge of Nebulous Ring.	Equatoreal Diameter of Planet.	Transverse Diameter of Ring.
1854.	″					
Nov. 15	40·655	...	...	...	...	...
Dec. 15	41·331	...	...	...	...	...
17	41·008	...	...	...	...	...
1855.						
Jan. 4	40·739	...	...	...	...	...
6	40·733	...	...	...	...	...
Apr. 20	41·205	...	...	...	17·708	...
Nov. 30	40·851	...	23·792	21·232	17·458	18·339
Dec. 5	41·324	34·486	26·101	20·995	17·829	...
14	41·068	...	25·474	...	17·531	20·442
15	41·443	34·657	25·913	21·725	17·773	19·025
16	40·812	34·699	25·834	21·350	17·716	18·504
23	41·118	34·642	25·917	21·605	17·611	18·991
24	40·564	34·760	26·191	21·519	17·687	18·291
27	40·412	...	25·832	21·508	17·572	18·110
	40·623	...	26·003	...	...	...
30	40·710	...	...	...	17·728	...
	41·090	...	26·083	...	...	...
1856.						
Jan. 9	40·483	...	...	...	...	...
Mean	40·893	34·659	25·714	21·419	17·661	18·814

"All the measures have been reduced to the mean distance of *Saturn* from the sun, but the results have not been corrected for the different inclination of the ring on the different days. It will be seen by this table that the discordances in the measures of the ring are very considerable, while, in the case of the planet, they are sufficiently small: for the ring they are generally not less than $0''{\cdot}5$, while the extremes amount to as much as $1''$, which is altogether intolerable with this instrument. After spending some time in anxious reflection upon the cause of these anomalous results, I consulted the measures of Mr. Lassell, and in these I found similar discordances. This circumstance induced me to discuss the measures with greater care, and the result has been that I think I have detected a certain period in the irregularities. The measures of two consecutive days do not agree; but those of three days and of nine are consistent. The measures of four days do not agree even in the case of Mr. Lassell's observations. I was

then led to inquire whether the results might not be explained by supposing the ring to be elliptic, and that by its rotatory motion around its primary, it presented alternately its major and minor axis. Having assumed the period of rotation assigned by Herschel, the results did not combine; but upon trying the period which a satellite would have if it were placed on the margin of the ring, I found, after applying a slight correction, that it satisfied the observations. Let T be the time of rotation of the ring; t, the time which elapses after an observed epoch of maximum; $\varkappa$, the difference of the two axes of the ring; we shall then have the mean diameter = the observed diameter $+ \varkappa \cos 2\left(\frac{2\pi t}{T}\right)$. If we denote by ω the angle which remains after whole circumferences have been described by the satellite, the correction will be $c = \varkappa \cos 2\omega$,* and it is evident that in a rotation there are two maxima and two minima. The value of T, which satisfies the observations best, is $T = 14^h{\cdot}428$ of sidereal time. From the difference between the maxima and the minima I have concluded that $\varkappa = 0''{\cdot}366$, and I have chosen for the epoch of minimum the 24th of December, at $4^h\ 10^m$ sidereal time, on which occasion the planet was so steady and well defined, that I entertain no doubt respecting the observation. The Table (C) gives the results of these calculations:—

Table (C).

Date and Hour.		Intervals In Time.	Intervals In Revolution.	Correction.	Diameter Observed.	Diameter Corrected.	Deviation from Mean $c-m$.
1855.	h	h m	r o		"	"	
Nov. 30	2·30	24+1 40	40½+23	+0 254	40·851	41·105	+0·118
Dec. 5	1·45	19+2 35	32 +76	−0 323	41·324	41·001	+0·014
14	3·00	10+1 40	17 +24	+0 245	41·068	41·311	+0·227
15	3·30	9+0 40	15 +81	−0 348	41·443	41·095	+0·108
16	3·30	8+0 40	13½+12	+0 334	40·812	41·146	+0·159
23	4·10	1+0 0	1½+65	−0 235	41·118	40·883	−0·104
24	4·10	...	...	+0 366	40·564	40·930	−0·057
27	3·40	3−0 20	5 +12	+0 334	40·412	40·746	−0·241
,,	4·10	3 0 0	5 +20	+0 280	40·623	40·903	−0·084
30	3·50	6−0 20	10 +32	+0 160	40·710	40·870	−0·117
,,	4·20	6+0 10	10 +45	0 000	41·090	41·090	+0·108
1856.							
Jan. 9	5·28	16+1 18	27½+20	+0 280	40·483	40·763	−0·224
					Mean ..	40·987	

"The column of corrected diameters presents a satisfactory agreement throughout a period, including forty revolutions of the ring. The outstanding differences are sufficiently small and of the order of the errors of observation, although there is a tendency to a positive excess in the earlier part. The correction for refraction was not, indeed, applied; and as the hour-angle frequently amounted to between 2^h and 3^h, some hundredths of a second might be gained

* The powers of the excentricity, which are higher than the second, are not taken into account.

in this way, but this would not suffice to make the differences altogether vanish. On the contrary, the excess observable may have the same origin as the anomalies which have characterised the measures of the celebrated astronomers who have formerly devoted their attention to this subject, that is to say, they may arise from real variations of the ring. It will be seen by the above measures that the means of the results of Tables (B) and (C) are not very different from those obtained by MM. Encke, Galle, and Lassell, but deviate from those of MM. W. Struve, Bessel, and Otto Struve, to an extent which far surpasses the probable limit of the errors of observation. It will then be desirable, in order to detect these variations, to compare the breadth of the actual rings with that which has been assigned by M. W. Struve. The mean obtained by M. Otto Struve (memoir above cited) from the observations of MM. W. Struve and Encke and those executed by himself is the following:—breadth of the exterior ring = 2″·440. From our measures we have determined the breadth of the ring to be 2″·911, taking into account the breadth of the division which has been found = 0″·402; the difference, therefore, amounts to 0″·475 (*a*).* On the 29th of December I was struck with the excessive apparent breadth of the exterior ring, which appeared to the eye broader than in the incomparable drawings of Lassell, Dawes, Otto Struve, and others. I was induced, therefore, to have recourse to actual measurement, and I found the total breadth from the

Exterior Border of A to the Interior Border of B to be	= 7″·512
Also Breadth of A	= 2·788
According to Struve	= 2·440
Difference ...	0·348

"The difference is sufficiently great to confirm what simple inspection had already led me to suspect.

"On the 27th of December the state of the atmosphere was very favourable. The two ansæ of the rings were measured. For the preceding ansa we found,—

	r	Reduced Breadth.
Double Breadth, including the wires =	1·178	8″·6055
— — — —	1·178	
— — — —	1·177	

(Revolution of the Screw = 15″·4667)

"For the following ansa,—

— — — —	1·196	8″·7506
— — — —	1·197	
— — — —	1·197	
According to Otto Struve		7″·15

* The third column in Table (B) gives the diameter of the middle of this division (*a*), upon which the middle of the wire was placed.

"The agreement of the individual measures would not justify us in attributing the difference to errors of observation; but without having recourse to any exaggeration, it may be seen that the ansæ are somewhat unequal, and that the breadth of the rings was greater than usual. From the above measures (Table B) it results that the interior diameter of the exterior ring A = 35″·061; W. Struve makes it = 35″·289; the difference is 0″·228; a quantity which is tolerable, but cannot be neglected in an object like this, which admits of being seen with such precision and facility, under a favourable condition of the atmosphere. It would follow, therefore, that the division has only in a slight degree shifted its position. If, then, there exists any variation, it must be in the breadth of the exterior ring. The ratio of the breadths of the two rings ought, consequently, to vary; and this circumstance might explain the discordance between the indications of Cassini and succeeding astronomers, in regard to the value of that ratio. In like manner the ratio of the dark space between the ring and the planet to the breadth of the ring ought to vary. The ratio of the former of these two quantities to the latter is,—

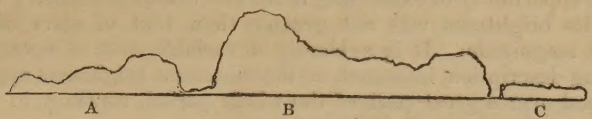
According to Otto Struve	0·49
Encke and Galle	0·57
W. Struve	0·64
Bradley	0·95
According to my last measures ...	0·53

"We should, then, be disposed to conclude that this system is subject to sensible variations. I should have wished, for this purpose, to wait for new observations; but I have thought that I ought no longer to delay communicating these results to astronomers, in the hope of engaging them to take exact measures of the ring while the opening has attained its maximum value; for it is to be feared that when the ellipse becomes more acute at the summit, the difficulties will become greater, and the results will be diminished. I offer the foregoing views merely as an essay which future observations may confirm or refute. It will be important henceforward to note exactly the time of observation and to indicate it along with the results obtained. The want of this indication has prevented me from profiting by several interesting observations. The practice of noting the time simply in so far as it is necessary for ascertaining the correction for refraction, is no longer sufficient.

"I shall conclude this paper by extracting from my Journal some observations relative to the appearance of the planet:—

"*24th December.* Atmosphere magnificent; charming image; the exterior ring A exhibits two well-defined streaks, and is, as it were, divided into three rings. The distance of the principal streak from the interior border of A being measured, was found equal to 1″·596. The second streak is very fine, and is distant

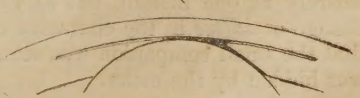
from the interior border of A at the utmost, 0″·5. These two streaks are seen not only at the ansæ, but all round the perimeter, except above the planet, where they are somewhat confused. The principal division *is not black*, but of the colour of the nebulous ring, C, that is to say, somewhat reddish. On other occasions it appeared bluish, indicating thereby changes of colour. The division is very nearly, but not quite so broad at the ansæ, as the wire of the micrometer (that is to say, nearly 0″·402). The difference of colour may be distinguished very well from the contrast of the two small points of black shadow which are seen on both sides of the summit of the planet, and of which one is broader than the other. The pole of the planet almost touches the ring A by a veritable mathematical contact. The ring B is seen with its light graduated, and somewhat brighter towards the interior edge. The curve here exhibited gives the approximate degree of the intensity of the light of the rings. The division between B and C is



seen well defined; its breadth equal to half the principal division. The ball of the planet presents a very brilliant band. Upon carefully examining the curvature of the lower margin of this band, we see that *it has not exactly the continuation of the nebulous ring;* so that the latter traverses the planet in a different part, and is projected beneath the bright band, and upon a more obscure zone. The pole of the planet is greenish grey, magnifying powers, 1000 and 500.

"*27th December.* The nebulous ring very well defined: more bluish than usual. *Enceladus* is very well seen in the field without making the planet go out of it. It is recognised by means of the position which it occupied in the preceding day. The globe scarcely touches the ring A; magnifying power, 1000 and 700. The principal division *is not black.*

"*31st December.* The globe exhibits a very small black filament under the border of A. This is verified by M. Galli (this young man has an excellent sight; he sees habitually, besides the seven stars of the *Pleiades*, three others the positions of which I have verified by means of the map). However, it is far from being under all the division. I would value this zone at one-eighth of a second. The shadow laterally is sufficiently developed, but it presents a protuberance at the summit (see the annexed figure). The nebulous ring well defined, but it traverses the planet in a part different from the inferior limit of the bright zone. (Upon observing at a distance with the telescope a



globe of crystal, in which is contained a jet of illuminating gas: the tinge of the crystal resembles that of the nebulous ring.)

"11*th January*, 1856. The two ansæ of the ring C are of different shades. This, on the side of the shadow, is blue; the other, reddish. The eye-pieces are achromatic, and have been changed expressly for that purpose."

On a new Variable Star. By J. R. Hind, Esq.

"On the evening of December 15th, 1855, I remarked in R.A. (1856) $7^h\ 46^m\ 33^s{\cdot}65$, N.P.D. $67°\ 37'\ 17''{\cdot}1$, an object shining as a star of the ninth magnitude, with a very blue planetary light, which I have never seen before during the five years that my attention has been directed to this quarter of the heavens. On the next fine night, Dec. 18, it was certainly fainter than on the 15th by half a magnitude or more. Since that date I have not had an opportunity of examining it till last evening, January 10th, when its brightness was not greater than that of stars of the twelfth magnitude. It is evidently a variable star of a very interesting description, inasmuch as the minimum brightness appears to extend over a great part of the whole period, contrary to what happens with *Algol* and *S Cancri*.

"The position given above was deduced by micrometrical comparisons with the principal component of the double star Σ 1158. The *variable* precedes $1^m\ 26^s{\cdot}53$, and is N. $7'\ 30''{\cdot}8$.

"*Mr. Bishop's Observatory*, 1856, *January* 11."

On the Occultations of the Star Antares (α Scorpii) in the Year 1856. By G. B. Airy, Esq., Astronomer Royal.

"In the year 1856 there will be several occultations of *Antares* by the moon visible in England, namely, on March 26, June 16, and August 10.

"It appears desirable that advantage should be taken of these phenomena for observation of the double character of the star.

"*Antares* is the only close double star of the first magnitude. Its duplicity was discovered, so far as I am aware, by Professor Mitchell, of Cincinnati, on the 13th of June, 1846,* with the large Munich telescope, mounted equatoreally in the Observatory there.

"After the double character of the star had been discovered by direct observation, it was remembered that, in the accounts of observed occultations of *Antares* by the moon, it had been remarked that the light of the star did not disappear or reappear entirely at one instant, but at two instants, with such a sudden change or *saltus* in the character of the light as made it probable that the small companion was seen for a time, while the large star was hidden by the moon.

* Silliman's *American Journal* (New Series), vol. i. p. 315.

"The small elevation of *Antares*, even when on the meridian, and the consequent confusion of image, make it impossible for us to see it as a double star in these latitudes. But the sudden change in the intensity of its light at occultation may probably be observed; and a careful register of the times of change of brightness and total disappearance, or of first reappearance and full brilliancy, will give elements for a computation of the relative position and distance of the two stars.

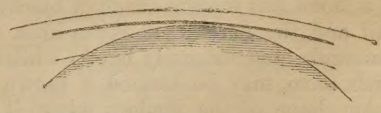
"*Royal Observatory*, 1856, *January* 1."

Observations of the Planet Saturn. By William Lassell, Esq.

"1855, *Nov. 27th.* $10^h\ 30^m$ G.M.T. Viewed *Saturn* with powers 430 and 565. Sky hazy, with hoar-frost. Planet continually disturbed by passing haze, and observation very tantalising. The shadow of the ball is seen all along the southern limb, extending into the division of the ring, and is lost in it. The ball evidently does not itself extend up to the division of the ring. The shadow seems quite flat-topped, conveying the idea that its presumed roundness is lost in the division; and if the extreme south limb of the shadow does really extend over the division, it is so faint as not to be distinguishable on the darker ring A.

"*Nov. 29th.* *Saturn*, about 11 P.M. was surveyed some time, but the air was worse than on the 27th. My impressions of the form and position of the shadow of the ball and the encroachment of the latter on the ring, are the same as on that occasion. On the 27th I noticed that the preceding arm of C was brown and the following grey.

"*Dec. 6th.* The division of the ring with power 565 seems quite uninterrupted all round. At the southern part the shadow of the ball comes up to it, and is lost in it; but, indeed, the southern limb of the planet becomes gradually so dark, that at its very edge it cannot be distinguished from shadow; and it is only by completing in imagination the symmetry of the ball in that part that an opinion can be formed, whether it touches the division itself, or whether there is an exceedingly narrow portion of shadow between. The sort of osculation which takes place between the shadow and the division of the ring, or between the dusky limb of the ball and the division of the ring, analogous to the *cornering* of the shadow I observed at Malta, is still plainly visible, but is to me without satisfactory explanation.

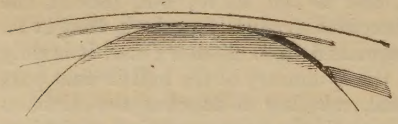


"*Dec. 12th.* Similar observations to the above were repeated.

"*Dec. 21st.* Circumstances rather unusually fine. Viewed *Saturn* about 10 P.M. with power 565. There is a very slight cornering of the shadow (?) at the south limb. The extreme south limb of the planet touches, I believe, the division. I cannot see

that the shadow is visible there beyond the limb, and there is certainly no encroachment of either shadow or ball upon ring A. Indeed, from the present greater elevation of the sun than of the earth above the plane of the ring, there ought to be no shadow visible. The dark ring is remarkably well shown, and certainly reaches fully half-way between the inner edge of B and the planet's limb. It is well defined at its interior edge, and the interior edge also of B is very sharp. The ruddy belt on the planet is striking, fading, however, very much away towards the limbs, which are well seen through the dark ring. The brownish yellow of the ball generally, south of the belt, and the very dark south pole, are strongly marked. The aperture of the telescope was reduced to 20 in., but the light was sensibly less, without a greater precision of image. Now that the ring is so widely open, the distance from the inner edge of B to the planet seems less than my drawings, made in October 1852 at Malta, would indicate, in proportion to the breadths of the bright rings. The bit of sky on each side of the ball is well contrasted in blackness with the dark ring, notwithstanding the proximity of the almost full moon.

"1856, *Jan. 7th.* 10 P.M. Wind east, light, and the sky very variable; generally, indeed, cloudy, but with momentary glimpses of good vision, though I doubt if the atmosphere was ever really clear. Viewed *Saturn* for some time with powers 430 and 565, watching for the finest glimpses. I am satisfied there is no shadow visible south of the southern pole, and that the south pole enters the division of the ring, passing, indeed, nearly, if not quite, through it. I receive the impression of a flattening of the ball there, or as if a bit of it were sliced off. I do not see that the ball reaches at all beyond the division, and I should best represent my conviction by saying that the southern edges of the ball and division were coincident. The shadow of the ball upon the ring is



plainly seen as a narrow black line south, following the ball, principally upon the ring B, not extending to A, and faintly seen, or imagined to be seen, across C.

"The division of the ring, generally termed black, is, however, of a much less intense shade of blackness than the shadow of the ball and the portion of sky within ring C, which are both preeminently and intensely black, whilst the division, in comparison with them, may be termed a brown or greyish black. The angular shape of the shadow, where it touches the division, remains as in former apparitions, and also something like a notch, even on the preceding side, where there is no shadow visible. Observation very troublesome, by reason of the haze or scud constantly passing and reducing the brightness of the planet down to that of a star of the fifth magnitude; and, indeed, frequently rendering it invisible to the naked eye.

"These observations, especially touching the encroachment of

the ball upon the ring, have been made in some measure to satisfy the query of Professor Secchi, in his paper in No. 982 of the *Astronomische Nachrichten*. They do not appear to afford any evidence of oscillation of the plane of the ring. Neither have I been able to detect any want of symmetry in the form of the division. But the sky during the latter part of the autumn and winter has been remarkably unfavourable for delicate scrutiny.

"In addition to the above observations of *Saturn*, I may mention a

Remarkable Configuration of Stars

found on pointing the telescope in the neighbourhood of *Uranus*. Approximate place, hastily and rather rudely taken, R.A. $3^h\ 13^m$, P.D. 72° 50′. No opportunity was afforded of measuring any of the stars, or even correctly ascertaining their magnitudes. They appeared thus in the field of view with power 260:—

"The four principal stars are very bright, and of very nearly equal magnitude. The companions of the three forming the equilateral triangle are very minute, so as to form similar and extremely unequal double stars. The south following companion is a delicate object even with 24 inches' aperture. Some relation amongst them is suggested by their remarkable juxtaposition.

"*Bradstones, 9th January*, 1856."

On Occultations of Stars by Saturn. By M. Winnecke.

(*Extract of a Letter to the Astronomer Royal.*)

"You will excuse my troubling you with a remark on the subject of your note in the December Number of the *Monthly Notices*, relative to the occultation of a star by *Saturn*, which was pointed out in No. 920 of the *Astronomische Nachrichten*. I hardly think that the conjunction will result in a real occultation, since the ephemeris of *Saturn*, published in the *Nautical Almanac*, has given the declinations in recent years with far greater accuracy than the astronomical ephemeris of this place has done. Assuming the *Nautical Almanac* as the basis of computation, we find the geocentric declination of the centre of *Saturn* for the instant of conjunction in right ascension with the star $7\frac{1}{2}^m$ to be:—

$$\delta\ \text{h} = +22°\ 14'\ 6''$$

"The American Ephemeris gives nearly the same value;—

$$\delta\ \text{♄} = +22^\circ\ 14'\ 9''$$

"The apparent declination of Lalande 13545 is, however,—

$$\delta\ * = +22^\circ\ 14'\ 26''\ \text{(Bessel's Zone 279, Lal.)}$$

"So that, probably, there will be only a close approach to an occultation. On the 13th of November, 1854, the sky was clear here. *Saturn*, as might be expected from the meridian observations of M. Bruhns (*Ast. Nach.* 922), remained to the south of Lal. 9362, without occulting it. It was very striking, however, on the occasion of this near conjunction to remark the faint light of this star of the 7.8 magnitude, compared with the light of *Titan*, which, it must be admitted, was farther removed from the planet. The star shone so feebly that it seems doubtful whether, apart from all other considerations, a star of the eighth magnitude would, from this circumstance alone, be visible in the space between the ring and the body of the planet.

"By extending my researches to the next year I have arrived at the remarkable conclusion, that, on the 29th of June, 1857, *Saturn* will, in all probability, almost centrally occult δ *Geminorum*, a star of the third magnitude. If we assume the apparent position of the star for this day to be,—

$$\alpha = 7^h\ 11^m\ 35^s{\cdot}20 \qquad \delta = +22^\circ\ 14'\ 37''{\cdot}0$$

the different ephemerides assign for the moment of conjunction in right ascension,—

		h	m					°	′	″
Berliner Jahrbuch, 1857, June 29,		11	47	G.M.T.	Geoc.	Dec.	♄ =	+22	14	48·0
Nautical Almanac	...	11	53	...	...			+22	14	34·5
American Ephemeris	...	12	3	...	...			+22	14	37·9

The Polar Semi-diameter of *Saturn* is 7″·9 (American Ephemeris).

"It is, indeed, very much to be regretted that a phenomenon of such rare occurrence, and one which might furnish important indications relative to the nature of the divisions of the ring, the transparency of the obscure ring and the perhaps oscillatory dimensions of the Saturnian system, can hardly be well observed. The position of the sun for the instant of conjunction is, in fact, $\alpha = 6^h\ 35^m$, $\delta = +23^\circ\ 13'$. Notwithstanding this unfavourable circumstance, it may be hoped that in consequence of the brightness of the star, this phenomenon, although invisible to us, may be observed in the daytime by the aid of the powerful telescopes of the Observatories of Cambridge, U.S., and Washington.

"The rarity of the phenomenon is still further enhanced by the circumstance that δ *Geminorum* is a double star. If we regard the observation of the elder Herschel as probably erroneous, it will be found that the remaining measures do not exhibit any pronounced relative movement.

"Struve's data for the star are:—

Magnitude 3^m, $8^m{\cdot}2$ Dist. = $7''{\cdot}13$ Position Angle 197° 39′

whence it follows that the companion star precedes the principal star by $2''{\cdot}2$, and is farther south than it by $6''{\cdot}8$. If we now assume that the place given in the *Nautical Almanac* is exact, it results that the centre of *Saturn* will pass *between* both stars. Galle has found by calculations based on the doctrine of probabilities, that, taking into consideration all the stars down to the ninth magnitude, inclusive, *Saturn* will occult one of such stars only once in every 181 years. If we now assume, in round numbers, the number of these stars to be 200,000 (Humboldt, *Cosmos*, Band iii. s. 146), which will not be far from the truth, and estimate, according to the same authority, the number of stars included between the first and third magnitudes to be 275, it follows, upon the supposition of an uniform distribution of stars in the celestial vault, that *Saturn* occults a star of one of these three classes of magnitude only once in 130,000 years.

"The above number assigned by Galle is, however, liable to some objections. I have not quite at hand the work in which it is deduced, but I rather think that it holds good only for an observer situate on the sun; whence it would follow that if we consider an inhabitant of the earth, the number must be diminished; that is to say, the occultations by *Saturn* must be more abundant. At all events, many thousands of years will elapse before an equally bright star will be again occulted by *Saturn*, and more especially a double star.

"If this matter should appear of sufficient importance to you, I beg of you to have the goodness to communicate it to the Royal Astronomical Society.

"*Berlin, January* 28, 1856."

Places resulting from Observations of Recently Discovered Small Planets made with the Transit Circle, at the Royal Observatory, Greenwich, during the month of December 1855.

Iris.

Greenwich M.S. Time.	R.A.	N.P.D.
h m s	h m s	° ′ ″
1855, Dec. 6 13 2 14·9	6 3 13·06	67 3 48·45
12 12 32 26·7	5 56 59·33	67 31 26·82
13 12 27 26·2	5 55 54·63	67 36 6·41
18 12 2 20·6	5 50 27·62	67 59 25·31
21 11 47 19·4	5 47 13·62	68 13 17·99

Parthenope.

Greenwich M.S. Time.	R.A.	N.P.D.
h m s	h m s	° ′ ″
1855, Dec. 12 9 27 47·6	2 51 49·88	80 21 43·13
15 9 14 40·5	2 50 30·36	80 19 6·55
18 9 1 49·6	2 49 26·99	80 15 16·37
19 8 57 35·1	2 49 8·41	80 13 36·01

Proserpine.

Greenwich M.S. Time.	R.A.	N.P.D.
h m s	h m s	° ′ ″
1855, Dec. 6 11 30 47·1	4 31 30·31	65 42 57·74
19 10 27 16·5	4 19 4·49	66 0 43·44

Psyche.

Greenwich M.S. Time.	R.A.	N.P.D.
h m s	h m s	° ′ ″
1855, Dec. 15 10 13 29·8	3 49 29·31	74 42 12·09
18 9 59 49·6	3 47 36·52	74 44 32·37
21 9 46 19·6	3 45 55·98	74 46 8·70

The north polar distances are corrected for refraction, but not for parallax.

In one or two cases the identity of the object observed for the planet is not quite certain.

Observations of the Solar Spots in the Year 1855. By M. Schwabe.

	Number of Groups.	Days of no Spots.	Days of Observation.
January	4	5	21
February	3	1	20
March	6	0	20
April	4	12	29
May	5	15	31
June	2	12	28
July	2	27	30
August	1	24	30
September	1	25	27
October	5	6	30
November	3	9	17
December	2	10	21

"The above catalogue indicates 313 days of observation and 38 observed groups of spots, which I could perceive with the $2\frac{1}{2}$-foot Fraunhofer and a magnifying power of 40; with the $3\frac{1}{2}$-foot instrument and a magnifying power of 42, and with the 6-foot instrument and a magnifying power of 64, I remarked 41 additional small groups, so that, if I retained my earlier system, I should have registered 79 groups. On 146 days the sun, when viewed with the $2\frac{1}{2}$-foot refractor, appeared free of spots. The spots were always small; their diameter never exceeded 17″; only a few exhibited any appearance of a penumbra around them. There were very few penumbræ without a nucleus. On the other hand, the whole surface of the sun, especially from October, appeared to be diversified with furrows and pores, which gave it a marble aspect.

"On several cloudy days, when I could observe the sun with the most transparent screen-glass, or even without a glass at all, the light of the sun's border appeared so remarkably faint, that the effect can only be satisfactorily accounted for by a light-enfeebling solar atmosphere.

"No faculæ were visible on the sun's disk.

"*Dessau, December* 31, 1855."

M. Schwabe has prosecuted observations of the solar spots without interruption since the year 1826, and has found that the number of groups is subject to a periodic recurrence. The following table exhibits the results of his observations for each succeeding year:—

Year.	Days of Observation.	Days of no Spots.	New Groups.
1826	277	22	118
1827	273	2	161
1828	282	9	225
1829	244	0	199
1830	217	1	190
1831	239	3	149
1832	270	49	84
1833	247	139	33
1834	273	120	51
1835	244	18	173
1836	200	0	272
1837	168	0	333
1838	202	0	282
1839	205	0	162
1840	263	3	152
1841	283	15	102
1842	307	64	68
1843	312	149	34
1844	321	111	52

Year.	Days of Observation.	Days of no Spots.	New Groups.
1845	332	29	114
1846	314	1	157
1847	276	0	257
1848	278	0	330
1849	285	0	238
1850	308	2	186
1851	308	0	151
1852	337	2	125
1853	299	3	91
1854	334	65	67
1855	313	146	79

In 1852, Dr. Wolf, of Berne, by an examination of old documents, obtained a confirmation of the period deduced by M. Schwabe. He also remarked that the period of the mean annual value of the diurnal variation of the magnetic needle in declination coincides with the period of the solar spots. Dr. Lamont, who first remarked the periodicity of the magnetic phenomenon, inferred that its variations recurred in intervals of about $10\frac{1}{2}$ years. Dr. Wolf, however, found that a period of 11·11 years would more satisfactorily represent the observations.

The following synopsis of the magnetic variations from 1835 to 1850, both years included, is extracted from a paper by Dr. Lamont on the subject. The results in the first column have been deduced from the *Göttingen Observations;* those in the second have been established by Dr. Lamont himself at the Munich Observatory:*—

	Mean Diurnal Variation in Declination of the Magnetic Needle.		Mean Diurnal Variation in Declination of the Magnetic Needle.
	′		′
1835	9·57	1841	7·82
1836	12·34	1842	7·08
1837	12·27	1843	7·15
1838	12·74	1844	6·61
1839	11·03	1845	8·13
1840	9·91	1846	8·81
1841	8·70	1847	9·55
		1848	11·15
		1849	10·64
		1850	10·44

Dr. Lamont has also shown that the observations of Colonel Beaufoy from 1813 to 1820, and the earlier observations of Gilpin and Cassini, indicate the same period of variation.

* Poggendorf's *Annalen*, vol. lxxxiv. p. 572.

Occultation of η Virginis, Mag. 3½, observed by Capt. Shadwell, R.N., January 26th, 1856.

		h	m	s	
Immersion	... ...	17	56	32·1	Portsmouth Mean Time.
Emersion	... ...	19	10	59·7	

Observations very satisfactory.

R.N. College, Portsmouth.

Observations of Venus near her Inferior Conjunction.
By Lieut. Brodie.

"I took much interest in closely watching *Venus* for some little time preceding its inferior conjunction, and have several sketches in the Observatory book of its relative appearances. I was unfortunately away from home on the day of the inferior conjunction, but my last observation was on October 4th, four days after the conjunction, when the following entry occurs in the Observatory book:—

"Observed *Venus*, which has passed its inferior conjunction four days; it is very finely crescented, indeed, and its definition very sharp and clear, the horns extending fully a semidiameter of the planet.

	°	′
The Distance of the Planet from the Sun at the time of this Observation was	9	45
Distance at Time of Conjunction*	8	15
Δ	1	30

At the close of the meeting the President made some remarks on the increasing importance of the subject of variable stars and the desirableness of multiplying observations of those interesting objects. This was a branch of astronomy which was more especially adapted to the amateur, since a good telescope of moderate power was all that was required for the purpose of observation. M. Argelander, who had distinguished himself by his researches on the periods of variable stars, had employed a method of investigation which was not only very effective in detecting nice gradations of light, but also possessed the advantage of being easily practised by any person who was desirous of prosecuting obser-

* Mr. Brodie, from a hasty reading of the statement attributed to Mr. Airy, at page 232, vol. xv. of the *Monthly Notices*, was led to suppose that the words there used had reference to *daylight* observations of *Venus;* and he mentions that it was for the purpose of testing the remark of the Astronomer Royal, as thus interpreted by him, that he was induced to observe the planet so near the time of conjunction. It will be seen that the words of Mr. Airy plainly refer to the observations of *Venus when the sun is beneath the horizon*, the only time when there exists any probability of discovering a faint crepuscular light in the planet indicative of an atmosphere, the sole object for which the observation of the planet near its inferior conjunction was originally suggested.—Editor.

vations of this kind. It consisted in selecting a few small stars in the immediate vicinity of the variable object, and employing them as standards of comparison for the purpose of ascertaining the quantity of light which the star emitted while in the course of passing through its successive stages of brightness. It is manifest that the results obtained by this method would be independent of the state of the atmosphere, since any fluctuation arising from this cause would equally affect both the variable star and the star of comparison. It would be, of course, indispensable to its successful application that the stars selected for comparison should be of different magnitudes, and that their light be found by attentive observation to be sensibly steady.

The Hypothesis of Otto Struve respecting the Gradual Increase of Saturn's Ring, discussed with reference to the Manuscripts of Huyghens and the Accuracy of modern Observations. By Professor Kaiser.*

In this memoir Professor Kaiser examines the grounds of the hypothesis recently advanced by M. Otto Struve relative to the existence of a continual variation of *Saturn's* rings. According to that hypothesis, while the diameter of the planet and the exterior diameter of the outer bright ring have constantly preserved the same values, the interior diameter of the inner bright ring has been constantly diminishing; consequently, the system of bright rings is gradually increasing in breadth, and thereby approaching nearer and nearer to the body of the planet.

The author, after some preliminary remarks, gives an exposition of the numerical results upon which M. Otto Struve founded his hypothesis. Assuming that the exterior diameter of the outer ring has always exceeded the diameter of the planet by 22″·20, M. Struve found for the breadth of the rings, the breadth of the dark space included between them and the planet, and the ratio of the latter of these two quantities to the former, the results contained in the following table:—

Observer.	Year.	Breadth of Rings.	Breadth of Dark Space.	Breadth of Dark Space. / Breadth of Rings.
		″	″	
Huyghens	1657	4·6	6·5	1·41
Huyghens and Cassini	1695	5·1	6·0	1·18
Bradley	1719	5·7	5·4	0·95
Herschel	1799	5·98	5·12	0·86
W. Struve	1826	6·74	4·36	0·64
Encke and Galle	1838	7·06	4·04	0·57
O. Struve	1851	7·43	3·67	0·49

* De Stelling van Otto Struve omtrent het breeder worden van den Ring van Saturnus getoetst aan de handschriften van Huijgens en de naaukeurigheid der latere waarnemingen door F. Kaiser. (Verslagen en Mededeelingen der Koninglijke Akademie van Wetenschappen. Deerde Deel, Amsterdam, 1855.)

The author remarks that of the seven determinations contained in the preceding table there is not one which does not tend to support the hypothesis of M. Otto Struve, that during the last two centuries the ring has been continually increasing in breadth. He adds, however, that if the probable errors of these determinations should exceed in magnitude the variations which they are supposed to undergo, any conclusion deduced from them must be considered as doubtful. It is true that the previous researches of M. Otto Struve afford no reason to suppose that he would have allowed his judgment to be warped by any equivocal considerations; but still it cannot be denied that a preconceived idea may lead the most candid inquirer astray in his reasoning. Moreover, the proneness to exaggerate the accuracy of observations is so common that M. Otto Struve may not impossibly have been led to commit a similar mistake in the course of his investigation.

The author accordingly proceeds to examine the method by which M. Struve arrived at the results contained in the foregoing table. He remarks that hitherto no astronomer had formally called in question the hypothesis of M. Struve, but that several early drawings of the planet had been cited, which, in so far as they might be considered trustworthy, appeared in some instances to confirm the truth of that hypothesis. He is of opinion, however, that these drawings were in all probability founded on the descriptions and drawings given by Huyghens, who devoted more attention to the appearance of the planet than any of his contemporaries, and that, consequently, they cannot be considered as furnishing any independent confirmation of the measures of the Dutch astronomer. Perhaps there may not seem sufficient grounds for the opinion here expressed by Professor Kaiser. There can hardly be any question, however, with respect to the inadequacy of those early sketches in deciding a question of such delicacy as that raised by the researches of M. Otto Struve.

Alluding to the wishes expressed by the Council of this Society that the object-glasses of Huyghens, in the possession of the Royal Society, should be remounted for the purpose of ascertaining the exact influence of irradiation in observations with them, the author has the following remarks:—

"Although I quite agree in the desirableness of ascertaining with greater precision the meaning which ought to be attached to the words of Huyghens, still I cannot refrain from expressing my surprise at the way in which the Royal Astronomical Society of London have proposed to effect this object. The object-glasses of Huyghens, which are preserved by the Royal Society of London, have focal distances of 210, 170, and 120 feet. These glasses were not executed by Christian Huyghens, as has been erroneously supposed in foreign countries, but by his brother Constantine Huyghens. They are all much larger than the largest of the glasses with which Christian ever observed the planet *Saturn*, and more especially they are very large compared with the glasses with which he executed the observations which have formed the

groundwork of M. Struve's theory. Whenever, then, the Saturnian system is observed with the glasses in the possession of the Royal Society, it ought certainly to exhibit a different appearance from that indicated by the descriptions and drawings of Huyghens, even although it had not in reality undergone any change, and therefore are such observations incapable in the slightest degree of deciding the question with respect to the variability or constancy of *Saturn*."

It is plain from the foregoing passage that the author labours under a totally erroneous impression with respect to the object which was proposed to be effected by the remounting of one of the Huyghenian object-glasses, in the possession of the Royal Society. It was, we need scarely mention, for the purpose of ascertaining the effect of irradiation, not upon Huyghens' observations of *Saturn* and his rings, but upon those of Bradley, which were executed with one of Huyghens' telescopes belonging to the Royal Society, that the idea of remounting the object-glass employed in those observations was at one time entertained in this country.*

Huyghens, in all his works, has only alluded twice to the dimensions of *Saturn's* rings, and on both occasions in very brief terms. In his *Systema Saturnium*, published in 1659, he remarks that the space between the ring and the body of the planet is equal to, or even a little greater than, the breadth of the ring ("latitudinem spatii inter annulum globumque *Saturni* interjecti æquare ipsius annuli latitudinem vel excedere etiam)." His next allusion to the subject is contained in his *Cosmotheoras*, which was first published in 1698. On this occasion he asserts that the space between the ring and the planet is exactly equal to the breadth of the ring ("vacuum spatium inter utrumque interjectum eandem quam annulus latitudinem habebit").

The mode by which M. Otto Struve determined the ratio of the interval between the ring and the planet to the breadth of the ring in both these instances is worthy of remark. According to his original assumption, the distance from the edge of the planet's disk to the exterior edge of the ring amounted constantly to $11''{\cdot}1$. If the dark space between the planet and the ring *appeared* exactly

* The expression of the author, "in order to ascertain what aspect *Saturn*, in his present condition, would have exhibited to Huyghens," ("obdat men, omtrent het voorkomen waaronder *Saturnus* in zijn' tegenwoordigen toestand door Huijgens zoude zijn waargenomen, zoude worden ingelicht") does not convey the exact meaning of the words used in the Annual Report of the Council of this Society, which were these:—"It appears, however, that to give these observations their full value, it will be desirable that observations on *Saturn* be repeated with Huyghens' object-glasses, still preserved, it is understood, in the care of the Royal Society" (*Monthly Notices*, vol. xiii. p. 129). The Huyghenian object-glass which Bradley employed in his observations of *Saturn* had been lent by the Royal Society to his uncle Pound; it had a focal length of 123 feet. This was the object-glass which the British Association at one time contemplated re-mounting, for the purpose of testing Otto Struve's theory, but the idea appears to have been subsequently abandoned. (See Brit. Assoc. Rep., 1852, p. xxxiii.; Rep. 1853, p. xxiv.) It may be remarked, that the oversight which the author has committed in this instance does not affect any of the conclusions at which he has arrived in the course of his discussion.

equal to the breadth of the ring, it must *in reality* have exceeded the latter quantity, in consequence of the effect of irradiation, and, therefore, must be somewhat greater than 5″·55. Supposing the irradiation to amount to 0″·5, M. Struve hence concluded that in 1657, when the breadth of the space between the planet and the ring was considered by Huyghens somewhat to exceed the breadth of the ring, the former of these spaces must have measured *very nearly* 6″·5, and the latter 4″·6; and he fixes upon these as the definitive values for the epoch to which they refer, whence he derives 1·4 for the ratio of the two quantities. With respect to the second epoch, the reasoning of M. Otto Struve is certainly objectionable. Proceeding upon the assertion of Huyghens, that the space between the planet and the ring, instead of being greater than the breadth of the ring, as it was in 1657, was now exactly equal to the latter, he hence concludes that the breadth of the dark space must, therefore, be less than 6″·5. But, he remarks, it must exceed 5″·5, on account of irradiation. The mean of these two quantities is 6″, which he lays down as the breadth of the dark space for the second epoch. This leaves 5″·1 for the breadth of the ring, whence he obtains 1·18 for the ratio of the two quantities corresponding to the year 1695.*

The author maintains that the reasoning by which these results have been deduced cannot stand the test of strict discussion. With respect to irradiation, he is of opinion that it depends upon the eye of the observer, that its effects are of a vague and contradictory character, and that the safest course generally is to take no account of its influence in astronomical observations. We extract the following passage from his remarks upon this subject:—

"Granted, however, that an irradiation exists, in virtue of which luminous objects appear enlarged when projected upon a dark ground, still it by no means follows that irradiation must cause dark objects projected upon a light ground to appear too small. If we assume that the amount of irradiation is the same, whatever be the magnitude of the dark object, we should then fall

* It will be seen that in determining the breadth of the dark space for the first epoch, M. Otto Struve adopted a *definite* value of irradiation; but he took no account of the remark of Huyghens, that the dark space *somewhat exceeded* the breadth of the ring. On the other hand, his investigation of the breadth of the dark space for 1695 is founded upon an *indefinite* value of irradiation and upon the supposition that the words of Huyghens, "vel excedere etiam," *were duly taken into account in determining the value of the space for* 1657. The results for the two epochs are not, therefore, comparable: first, because a similar view of the influence of irradiation was not taken in both investigations; secondly, because the principles of ratiocination in the two cases are in contradiction with each other. In fact, if the value of irradiation be assumed equal to 0″·5 in both cases, the breadth of the dark space must have been somewhat greater than 6″·5 in 1657, and must have been exactly equal to that quantity in 1695, or, in other words, the ratio of the two quantities would have amounted to a little more than 1·41 at the former of these epochs, and to exactly 1·41 at the latter epoch, instead of 1·41 and 1·18 respectively, according to M. Otto Struve.

into the absurd conclusion that very small dark objects are changed by irradiation into objects of a greater degree of brightness than that of the ground upon which they are projected, and no relation between the magnitude of the object and the quantity of irradiation can be assumed so long as we possess absolutely no proof of the existence of such a relation."

The author then proceeds to remark that the observations of Huyghens show that dark objects when viewed by him did not appear diminished by irradiation to the extent of $0''.5$. It appears from his Journal that he repeatedly observed the shadow of one of *Jupiter's* satellites upon the body of the planet. In general, the satellites subtend an angle of $1''$, and the apparent diameter of the shadow in any case must, consequently, be somewhat less than that quantity. Upon this ground the author contends that if the effects of irradiation had amounted to $0''.5$, the shadows of the satellites must have been totally invisible to Huyghens,—a result which is at variance with the records of his Journal. He concludes that the safest course is to reject all consideration of the influence of irradiation, and, assuming with M. Otto Struve that in 1657 the dark space between the planet and the ring appeared to Huyghens to be equal to the breadth of the ring, the ratio of the two quantities would be not 1·41, but simply 1·0. He, moreover, maintains, upon the ground of the unsatisfactory nature of M. Otto Struve's reasoning, with respect to the influence of irradiation upon the observations of 1695, that the ratio of the dark space to the breadth of the ring for that epoch must also be considered as equal to unity.

The author next gives an account of certain drawings of *Saturn* and his rings which have been found among the manuscripts of Huyghens, preserved in the University of Leyden. They are merely rough representations which Huyghens sketched out with his pen in one of his observation-books.* The earliest drawing refers to the appearance of the planet on the 27th of December, 1657; the latest to his appearance on the 24th of August, 1693. The total number of drawings amounts to twenty-three. Selecting those that appeared most worthy of confidence, the author carefully examined them for the purpose of ascertaining what light they were calculated to throw upon the subject of discussion. The following are the values of the ratio of the dark space to the breadth of the ring which he obtained for four distinct epochs by actual measurement of the drawings:—

	Breadth of Dark Space / Breadth of Rings.
1657, Dec. 27	0·72
1675, Dec. 8	0·95
1682, Oct. 16	1·01
1693, Aug. 24	1·24

* Professor Kaiser's paper contains copies of three of these drawings, referring severally to the appearance of the planet in the years 1657, 1675, and 1693.

It would appear from these results that during the lifetime of Huyghens the ring, instead of gradually increasing in breadth, was, on the contrary, growing narrower. The author, however, is of opinion that the observations of the Dutch astronomer are incapable of leading to any trustworthy conclusions on the subject.

"A mere comparison of the drawings of Huyghens with the most recent measures cannot," says he, "decide anything respecting the variability or constancy of the rings of *Saturn*. The latest measures of *Saturn's* rings, namely, those which have been executed by Mr. Main at Greenwich, in the beginning of the year 1853, assign 0·82 for the ratio of the dark space to the breadth of the ring. The drawings of Huyghens assign to this ratio all values included between 0·72 and 1·24. We might, therefore, by a comparison of the observations of Huyghens with those of recent times, make out that the ring was gradually becoming broader or narrower, according as we found either hypothesis convenient for our purpose; and we shall, therefore, do best by considering the observations to be totally unsuitable for an investigation such as that undertaken by M. Otto Struve."

After an examination of Bradley's and Herschel's observations of *Saturn* and his rings, the author next considers the modern observations, namely, those of W. Struve, Encke, Galle, Bond, Otto Struve, and Main. On the observations of the latter, which were executed in 1852–3, the author has the following remarks:—

"These measures have now furnished a result which for once is at variance with the hypothesis of Otto Struve. They assign 0·80 for the ratio of the dark space to the breadth of the ring, a value which exceeds the ratio obtained by W. Struve, as much as the ratio of Otto Struve falls below the latter. If, then, the measures of Otto Struve demonstrate that since the year 1826 the ring of *Saturn* has sensibly increased in breadth, the observations of Main, on the other hand, indicate an equal diminution of breadth for the same period. If we assume that for the smaller, and perhaps less perfect telescope of Main, the amount of irradiation is much greater than for the telescope of Pulkowa, the measures would be still more unfavourable to the hypothesis of Struve. Admitting that the irradiation in Main's telescope amounted to merely a quarter of a second, we should then obtain 0·95 for the ratio of the dark space to the breadth of the ring, a result agreeing with that which M. Struve deduced from the measures of Bradley, executed 134 years earlier, and also with the ratio indicated by the most complete drawing of *Saturn*, contained in the Journal of Huyghens, which was written 177 years previously. From the discordance between Otto Struve and Main we may form an opinion of the degree of confidence to which the measures and estimations of the seventeenth and eighteenth centuries are entitled."

The author concludes by asserting that there exists no reason

whatever for supposing that the compound ring of *Saturn* is gradually increasing in breadth.*

* Professor Kaiser's paper contains a variety of critical remarks upon the advantages and defects of the heliometer, the wire micrometer, and the double-image micrometer, in measures such as those relating to *Saturn's* rings. He is inclined to prefer the modified form of Airy's double-image micrometer suggested by Valz (*Monthly Notices*, vol. x. p. 160), and, with the view of testing the justness of the conclusion at which he arrived, he has ordered one of such micrometers of Mr. Simms, which he purposes applying to the telescope of the Leyden Observatory.

CONTENTS.

	Page
Fellows elected	49
Announcement respecting Vol. XXIV. of the *Memoirs*	ib.
Discovery of a New Planet, by M. Chacornac	ib.
On the Rings of *Saturn*, by Professor Secchi	50
On a new Variable Star, by Mr. Hind	56
On the Occultations of the Star *Antares* (*α Scorpii*) in 1856, by the Astronomer Royal	56
Observations of the Planet *Saturn*, by Mr. Lassell	57
On Occultations of Stars by *Saturn*, by M. Winnecke	59
Places resulting from Observations of Small Planets at the Royal Observatory, Greenwich, in December 1855	61
Observations of the Solar Spots in 1855, by M. Schwabe	62
Occultation of *η Virginis*, by Capt. Shadwell	65
Observations of *Venus* near her Inferior Conjunction, by Lieut. Brodie ...	ib.
Remarks on the Observation of Variable Stars, by the President	ib.
On *Saturn's* Rings, by Professor Kaiser	66

London :—Printed by G. Barclay, Castle St. Leicester Sq.